



Learning Ricardian Equivalence[☆]



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ABSTRACT

This paper tests whether subjects learn to comply with the Ricardian Equivalence proposition in a life cycle consumption laboratory experiment. Our results suggest that Ricardian Equivalence does not hold on average: tax changes have a significant and strong impact on consumption choice. Using individual consumption time series, the behaviour of 56% of our subjects can be classified as inconsistent with the Ricardian Equivalence proposition. Classifying subjects according to rules of thumb that best describe their behaviour, we find that subjects switch less to rules that theoretically violate Ricardian Equivalence in later rounds compared to earlier rounds. This implies that some subjects learn to comply with Ricardian Equivalence by changing their consumption strategy. However, the aggregate effect of taxation on consumption persists, even after eight rounds of repetition.

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1. Introduction

The question whether people behave in accordance with the Ricardian Equivalence proposition has been subject to debate among economists, ever since it has first been formulated by David Ricardo in 1820. Generally the evidence seems mixed: While the excellent survey by Seater (1993) suggests that the data support Ricardian Equivalence, many studies are less favourable.¹ However, one of the reasons for evidence against Ricardian Equivalence might be the difficulty to control for

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¹ See, e.g., Mertens and Ravn (2014), Mertens and Ravn (2013), Cloyne (2013), Shapiro and Slemrod (2003), Souleles (2002), Souleles (1999), Shapiro and Slemrod (1995), Bernheim (1987), Summers and Carroll (1987).

confounders in survey or register data. For instance the presence of progressive taxation, political uncertainty, liquidity constraints, heterogeneity in preferences and uncertainty, and other confounders lead to a violation of Ricardian Equivalence in theory, and hence it is hard to infer if Ricardian Equivalence would hold, were these confounders not present. Laboratory experiments may help in this case, as they allow to control for these confounders, or to manipulate them in a controlled way.

Previous experimental research has identified under what conditions Ricardian Equivalence can be expected to hold: Cadsby and Frank (1991) were first to experimentally explore Ricardian Equivalence, and find that the results generally confirm Ricardian Equivalence. Slate et al. (1995) reject Ricardian Equivalence in cases where there exists uncertainty about debt repayment. Di Laurea and Ricciuti (2003) also reject Ricardian Equivalence in treatments with income uncertainty. Finally, Adji et al. (2009) find supporting evidence for Ricardian Equivalence under lump sum taxation, but reject Ricardian Equivalence under distortionary taxation.²

While all these studies use overlapping generations (OLG) models as a theoretical basis for the experimental design, Luhan et al. (2014) and Geiger et al. (2016) study consumption behaviour in a multi-period life-cycle consumption setup with two main findings that are relevant for our study. First, Luhan et al. (2014) find that anticipated future changes in prices exert substantially smaller effects on current consumption than predicted by a life cycle model. Second, Geiger et al. (2016) focus on the so-called expectations channel and find that fiscal consolidation (e.g. tax increases) have contractionary effects on consumption.³

A question that has not yet been answered is whether people can learn to behave according to the Ricardian Equivalence proposition. Insights on learning behaviour are crucial to understand decision-making of individuals who are not perfectly rational. If people learn to improve their decision-making, decisions in otherwise equal circumstances will vary depending on how much learning has happened. Moreover, analysing learning behaviour is particularly important for laboratory experiments, where results are typically judged by how robust they are with respect to learning (Hertwig and Ortmann, 2001). This paper attempts to fill this gap, and answer the question whether people can learn to comply with the Ricardian Equivalence proposition.

The importance of learning has been recognised by a theoretical literature that analyses if individual learning allows to find the optimal consumption function in a life cycle setting. The conclusion from the pioneering work in Allen and Carroll (2001) is quite pessimistic. However, Yıldızoğlu et al. (2014) show that assumptions on bounded rationality and on adaptive expectations are perfectly compatible with sound and realistic economic behaviour, which can converge to the optimal solution. Brown et al. (2009) find in a life-cycle consumption experiment that subjects saved much too little at first, but learned to save close to optimal amounts after three or four life cycles of learning.⁴

To analyse learning effects we use a setup similar to Brown et al. (2009). The experimental environment is based on a (25 period) life-cycle consumption model, that is favourable for analysing Ricardian Equivalence in the sense that it is challenging enough to find the optimal consumption rule to observe learning behaviour.⁵

In our experiment, a Ricardian tax scheme is implemented as a tax cut in early periods of the experiment, followed by a tax increase of the same magnitude in later periods. Introducing such a tax scheme may increase the difficulty to smooth consumption for subjects.⁶ We therefore introduce two different taxing schemes, one that increases the difficulty to smooth consumption and one that decreases it relative to a control treatment with constant taxation. In this way we can analyse the effects of difficulty and Ricardian taxation separately. This is a novel approach with regard to existing experimental studies on Ricardian Equivalence.

Our first main finding is that Ricardian taxation *does* influence consumption decisions. A nonparametric analysis shows that deviations from optimal consumption appear to be larger with the tax scheme that increases the difficulty to smooth consumption compared to the one that decreases the difficulty. Overall, deviations from optimal behaviour are lowest in the treatment with constant taxation. This implies that both difficulty and Ricardian taxation affect consumption behaviour.

Using structural panel data methods to estimate consumption functions allows to quantify the effect that taxation has on consumption: our second main result is that a tax benefit in early periods increases consumption by about 21% of the tax benefit on average, while a tax increase reduces consumption by 25% of the tax increase. These results are robust to variations in the difficulty to smooth consumption.

Using the same structural estimation strategy on the individual level, our third main finding is that about 56% of the subjects in our sample do significantly react to tax cuts or increases, and thus do not behave according to the Ricardian proposition.

To explain these results, we formally analyse learning effects by comparing behaviour across the eight repetitions of the experiment. While subjects in our experiment appear to learn to improve their consumption decisions, our fourth main

² See Online Appendix B for a table summarising the existing experimental results on Ricardian Equivalence.

³ These findings imply that we can reasonably expect taxation to have an influence on behaviour in a life-cycle consumption experiment. However neither Luhan et al. (2014) nor Geiger et al. (2016) test whether subjects comply with the Ricardian Equivalence proposition.

⁴ More broadly, our research is connected to the literature on learning and strategy evolution (e.g. Arifovic, 1994) in macroeconomic models surveyed by Sargent (1993) or Evans and Honkapohja (2001). In particular, we relate to studies with focus on intertemporal choice problems (Lettau, 1997; Lettau and Uhlig, 1999).

⁵ We focus on individual learning rather than social learning, since the latter is difficult to implement in the laboratory (Brown et al., 2009).

⁶ See Section 2.1 for an example.

finding is that subjects do on average not learn to behave according to the Ricardian Equivalence proposition even after repeating the experimental life cycle eight times.

To understand individual behaviour, we then classify subjects' behaviour according to different rules of thumb that best describe their behaviour. We find that subjects increasingly change their consumption rules to rules that theoretically imply Ricardian Equivalence in later rounds compared to early rounds. This implies that some subjects learn to comply to Ricardian Equivalence by adjusting their consumption rules.

The remainder of this paper is structured as follows. Section 2 describes the experimental design and the underlying theory. Section 3 reports our results. Section 4 concludes.

2. Theory and experimental design

The experiment described in the following section is based on an adapted version of the life cycle model of consumption used in Meissner (2016). One experimental life cycle lasts for $T = 25$ periods. In order to assess learning effects, we repeat the experiment for a total of eight independent life cycles (rounds). In each period $t = (1, \dots, T)$, subjects decide how much to consume (c_t) and implicitly how much to save or borrow. There is no discounting, and no interest is paid on savings or debt.⁷ Period income y_t follows an i.i.d. stochastic process and takes the values of 120 or 250 with equal probability in each period. Subjects have to pay a lump sum tax τ_t in every period. The government's budget constraint requires the amount of total taxes to be collected during the experiment to equal ϑ . The subjects' intertemporal budget constraint requires period consumption plus period savings (a_{t+1}) plus period taxes to equal period wealth, which is defined as $w_t = y_t + a_t$. Period savings are allowed to be both positive and negative. Savings in the last period (a_{T+1}) must equal zero, which implies that remaining wealth must be consumed in that period. Subjects start with initial savings, $a_1 = 1000$.⁸ These two conditions ensure that the intertemporal budget constraint is binding, i.e. $\sum_{t=1}^T y_t + a_1 = \sum_{t=1}^T c_t + \vartheta$.

Induced preferences are given by a time-separable CARA utility function: $u(c_t) = 338[1 - e^{-\theta c_t}]$, where the coefficient of absolute risk aversion, θ , is set to 0.0125.⁹ We chose the parameters of our model in order to make the incentives for subjects to behave optimally as salient as possible. This requires sufficient curvature of the utility function around optimal consumption. Moreover, our parameterisation ensures that the payoff function is easy to understand and guarantees an average hourly wage that complies with the rules of the laboratory.¹⁰ Note that optimal consumption is not very sensitive to variations of θ around our parameter choice.

The subjects' objective is to choose consumption in every period to maximise the expected utility of life-time consumption. The decision problem subjects face at any period t can be written as:

$$\max_{c_t} E_t \sum_{j=0}^{T-t} u(c_{t+j}) \tag{1}$$

$$\text{s.t. } c_t + a_{t+1} + \tau_t = w_t, \tag{2}$$

$$a_1 = 1000, \quad a_{T+1} = 0, \tag{3}$$

$$\sum_{t=1}^T \tau_t = \vartheta. \tag{4}$$

With CARA utility, this optimisation problem can be solved analytically (Caballero, 1990; 1991). Optimal consumption in period t is equal to:¹¹

$$c_t^*(w_t) = \frac{1}{T-t+1} [w_t + (T-t)y_p - \mathcal{F}_t - \Gamma_t(\theta, \sigma_y)]. \tag{5}$$

$$\Gamma_t(\theta, \sigma_y) = \sum_{j=0}^{T-t} \sum_{i=1}^j \frac{1}{\theta} \log \cosh \left(\frac{\theta \sigma_y}{T-t+1-i} \right). \tag{6}$$

⁷ We explicitly abstract from interest, in order to not further complicate the experiment, and to control for exponential growth bias (Levy and Tasoff, 2015).

⁸ One often-stated reason for the violation of Ricardian Equivalence is borrowing constraints. In order to avoid a failure of Ricardian Equivalence by design, our model has no borrowing constraints. Implicit borrowing constraints, such as debt aversion (see Meissner, 2016), might have a similar effect. To rule out these effects, we endow subjects with a positive level of wealth at the beginning of the experiment.

⁹ CARA utility was chosen because this class of utility functions is defined in the negative domain. Why this is of importance will be explained later in this section. Using CARA preferences we connect to Caballero (1990, 1991) and other studies on experimental life cycle consumption/savings problems that also make use of CARA utility. See, for instance, Carbone and Hey (2004).

¹⁰ See Section 2.2 and Online Appendix D.

¹¹ See Online Appendix A for the derivation of optimal consumption.

$$\mathcal{T}_t = \sum_{j=0}^{T-t} \tau_{t+j} = \vartheta - \sum_{j=1}^{t-1} \tau_j. \quad (7)$$

In Eq. (5), y_p denotes permanent income, which is equal to the mean of the income process, i.e. 185. $\sigma_y = 65$ is one standard deviation of the income process. Eq. (6) is the term for precautionary saving.

Note that with respect to tax payments, optimal consumption only depends on the *sum* of current and all future tax payments \mathcal{T}_t . Therefore, a tax cut in period t will not affect current optimal consumption. This is because any tax cut must be followed by a later increase in taxes of the same magnitude to permit the government intertemporal budget constraint to hold. In the period after a tax cut, wealth will be higher compared to the same situation without a tax cut in the previous period. This higher wealth, in turn, is offset by the sum of current and future tax payments \mathcal{T}_t which increases by the same amount, leaving optimal consumption unchanged. This implies that the size and order of each of the single lump sum tax payments $\tau = (\tau_1, \tau_2, \dots, \tau_T)$ plays no role with respect to optimal consumption, as long as the sum of tax payments over the life cycle is kept constant. This is the definition of Ricardian Equivalence in our experimental environment.

In order to test Ricardian Equivalence, we vary the temporal structure of tax payments, while keeping the sum of taxes to be paid over the experimental life cycle constant. Since optimal consumption is not affected by this variation, subjects have no incentive to react to tax cuts or increases and we can directly compare consumption decisions under different tax schemes.

2.1. Treatments

The basic idea of a Ricardian experiment in our framework is a tax cut in early periods of the experimental life cycle that is financed by a tax increase in later periods (Seater, 1993). To isolate the effect of Ricardian taxation we first run a control treatment in which tax payments are kept constant at 120 in all periods ($\vartheta = 3000$). This treatment will be compared to treatments that resemble a Ricardian tax scheme specified in more detail below.

A potential concern in our experiment is that Ricardian taxation may influence the difficulty to smooth consumption. Consider the following two-period example: in each period income can take on the values 0 and 10 with equal probability. Suppose the income realisations $y = \{0, 10\}$ are observed in periods 1 and 2, respectively. If the government introduces a tax scheme $\tau^1 = \{-5, 5\}$, net income becomes $y^{net} = y - \tau = \{5, 5\}$. In this case smoothing consumption may appear to be easier with taxation than without taxation, because taxation smooths (net) income. On the other hand, if the government decides to do the opposite and asks for a tax scheme $\tau^2 = \{5, -5\}$, net income equals $y^n = \{-5, 15\}$ and smoothing consumption might appear more difficult with taxation than without taxation. Of course, taxation does not influence optimal consumption since lifetime income remains unchanged. In particular, the uncertainty of net income is not changed because taxation is deterministic.

In our experiment, differences in behaviour between the Control and the Ricardian treatment could arise from the increased level of difficulty to smooth consumption. It would be misleading to interpret this observation as evidence against Ricardian Equivalence.

To account for this, we design two Ricardian treatments that differ with respect to the difficulty to smooth consumption. This enables us to distinguish the effect of Ricardian taxation from the difficulty of smoothing consumption.

In the first Ricardian treatment (Ricardian 1) tax cuts in the beginning of each round are only given when subjects observe a *low* (i.e. $y_t = 120$) income realisation (see Table 1). Analogously, tax increases in the later periods of the experiments are only implemented when subjects observe a *high* (i.e. $y_t = 250$) income realisation. If subjects react to changes in net income, this treatment should be easier to play than the Control treatment, because this taxing scheme essentially smooths net income.

In the second Ricardian treatment (Ricardian 2) tax cuts in the beginning of each round are only received when subjects observe a *high* income realisation. Tax increases in later periods are only implemented when subjects observe a *low* income realisation. This makes net income less smooth and therefore may make it harder for subjects to smooth consumption. Table 1 shows the different tax schemes for one exemplary realisation of the income stream.

To ensure comparability between treatments, subjects in treatments Control, Ricardian 1 and 2 experience the same realisation of the income process, i.e. in any period and round during the experiment, subjects in these treatments receive the same (gross) income. Income realisations differ between rounds, to increase the robustness of our findings by ensuring that observed behaviour is not merely an artefact of one particular realisation of the income process. The timing of the incidence of the tax rates is unknown to subjects in these treatments, and varies with the stochastic income process. However, this does not introduce additional uncertainty because, as shown above, only the sum of taxes over one life cycle is relevant for optimal consumption. This sum is deterministic and kept constant across treatments.¹²

¹² One question in the quiz about the instructions aimed to test if this was understood by subjects (see Online Appendix E, paragraph “Taxes”). About 90% of subjects answered this question correctly, and most of the remaining 10% did choose not answer this question (there was no check if all subjects answered all questions).

Table 1
Experimental design of tax schemes for treatments.

Period	Income Realisation	Control		Ricardian 1		Ricardian 2		$c_t^*(w_t^*)$
		Taxes	Net Income	Taxes	Net Income	Taxes	Net Income	
1	120	120	0	0	120	120	0	98.53
2	120	120	0	0	120	120	0	95.87
3	120	120	0	0	120	120	0	93.09
4	120	120	0	120	0	120	0	90.19
5	250	120	130	120	130	0	250	93.35
6	250	120	130	120	130	0	250	96.67
7	120	120	0	120	0	120	0	93.32
8	120	120	0	120	0	120	0	89.79
9	250	120	130	120	130	0	250	93.70
10	120	120	0	120	0	120	0	89.74
11	250	120	130	120	130	120	130	94.19
12	250	120	130	120	130	120	130	98.97
13	250	120	130	120	130	120	130	104.13
14	120	120	0	120	0	120	0	98.89
15	250	120	130	120	130	120	130	105.02
16	120	120	0	120	0	120	0	98.79
17	120	120	0	120	0	240	–120	91.89
18	250	120	130	240	10	120	130	100.43
19	250	120	130	240	10	120	130	110.25
20	250	120	130	240	10	120	130	121.81
21	250	120	130	120	130	120	130	135.87
22	250	120	130	120	130	120	130	153.75
23	120	120	0	120	0	240	–120	134.99
24	250	120	130	120	130	120	130	173.91
25	120	120	0	120	0	240	–120	132.84
$E[(y - \mu_y)^2]$	4225		4225		4225		4225	

Source: One exemplary realization of the income stream of own experimental design.

To account for the possibility that subjects may nevertheless experience subjective uncertainty about the tax payments, which may affect their behaviour, we introduce an additional control treatment.¹³ In this treatment (Ricardian 3) the path of all future tax payments is known to subjects. Subjects are informed in the instructions that in periods 2, 4, and 6, taxes will be cut to 0. Taxes are announced to increase to 240 in periods 18, 20, and 22. Since the incidence of the tax payments is now fixed, using the same income realisations as in the previous three treatments may lead to a systematic difference in difficulty across the eight rounds. To prevent this, the income process is drawn individually for each subject in each round in this treatment. Therefore, a direct comparison across treatments is only possible for the treatments Control, Ricardian 1, and Ricardian 2. The regression analysis in Section 3.2 allows to compare behaviour across all four treatments conditional on income.

2.2. Experimental procedures

The experiment was programmed and conducted with the software z-Tree (Fischbacher, 2007). The experimental software is an adapted version of the software used in Meissner (2016).¹⁴ In the instructions, consumption was explained to the subjects as buying “points” by spending the experimental currency “Taler”, in which income was denoted (see Online Appendix D). The experimental currency was converted to points by the utility function specified above. Subjects were informed about the exact form of the utility function. Furthermore, they were given a graph of the function and a table with relevant function values. The advantage of framing consumption as buying points is that negative consumption can be explained as selling points in return for experimental currency.

At the beginning of the experiment, subjects were given time to read the instructions, which were then read aloud by the experimenter. After this, subjects completed a quiz (see Online Appendix E) about the content of the instructions. The correct answers to all questions were then read aloud before subjects started the actual experiment.

In each period of the experiment, subjects were asked to input consumption decisions in an interface that displayed period income, savings from the last period, wealth, and taxes. The interface showed the history of all previous decisions and relevant values, such as savings, wealth, taxes, the sum of taxes paid so far, and the number of purchased points and accumulated points. Before a consumption decision was submitted, subjects were informed about how it would translate into points and the amount of savings that would be available in the next period. After this information was displayed, subjects

¹³ This treatment is motivated by the finding in Geiger et al. (2016) that subjects do only dissave enough if the future tax path is shown to them. The results in Luhan et al. (2014) and Geiger et al. (2016) are however inconclusive about the consumption response to known future changes.

¹⁴ A screenshot of the experimental interface is provided in Online Appendix D.

had the opportunity to start over; that is, they could specify a different level of consumption and check its implications. In the final period of each life cycle, the program automatically spent that period's wealth minus taxes as consumption.¹⁵ Then, subjects were informed on a separate screen about the amount of points they purchased during the round. At the end of the experiment, two of the eight experimental life cycles were randomly chosen to be payoff relevant. After the actual experiment, subjects were asked to fill out a questionnaire that contained incentivised lottery choices, which assessed individual risk aversion.

Subjects' payoffs were determined by a pre-announced linear function of the amount of points purchased in the two relevant rounds. Subjects received a show-up fee of 5 Euro and earned 17.79 Euro on average. The minimum final earning including monetary incentives for elicitation of risk preferences was 5 Euro, the maximum 56.3 Euro, and the standard deviation was 10.18 Euro.

The experiment was conducted at the laboratory of the Technical University of Berlin. Subjects were recruited using ORSEE (see Greiner, 2004). A total of 176 subjects participated. Most of the subjects were undergraduate students in the field of economics or engineering. About one third of the subjects were female.

3. Data analysis

To identify the effect that taxation has on consumption, we employ two strategies. First, we directly compare deviations from optimal behaviour across treatments to identify treatment effects. Second, we run panel regressions to measure the effect of taxes on the deviation from optimal consumption.

3.1. Deviations from optimal behaviour

As a first step in analysing our experimental data, we examine deviations from optimal behaviour.¹⁶ Since subjects in the treatments Control, Ricardian 1, and 2 observe the same income realisations throughout the experiment, we can directly compare deviations from optimal consumption across treatments and rounds.¹⁷ Deviations from optimal consumption can be assessed with the following measure (see Ballinger et al., 2003):

$$m_1 = \sum_{t=1}^T |c_t^*(w_t) - c_t|, \quad (8)$$

where $c_t^*(w_t)$ is conditionally optimal consumption (depending on current wealth w_t), and c_t is observed consumption in period t . This measure is the sum of absolute deviations from conditionally optimal consumption for one subject and over one experimental life cycle. Indices for subjects and rounds are dropped to facilitate legibility.

In order to compare deviations from unconditionally optimal consumption, we also calculate the following measure:

$$m_2 = \sum_{t=1}^T [u(c_t^*(w_t^*)) - u(c_t)], \quad (9)$$

where $c_t^*(w_t^*)$ denotes unconditionally optimal consumption at period t as a function of optimal period wealth w_t^* . This measure can be interpreted as the utility loss that results from suboptimal consumption. With this measure we can assess the effect of Ricardian taxation on welfare in our experimental environment.

Fig. 1 shows the medians of the measures m_1 and m_2 by treatments and rounds.¹⁸ At first glance subjects appear to perform best in the Control treatment. Subjects in the Ricardian 2 treatment have higher deviations from optimal consumption and a higher utility loss compared to subjects in the Control treatment. Subjects in the Ricardian 1 treatment seem to be somewhere between the Control and Ricardian 2 treatments.

This intuition can be confirmed by examining the total effect; that is, the measures m_1 and m_2 averaged for each subject over the eight rounds of the experiment. For both measures, subjects perform significantly better in the Control treatment compared to subjects in Ricardian 1 (p-values from a Mann-Whitney U-test are provided in Table 2). Subjects in the Ricardian 2 treatment have significantly higher absolute deviations from optimal consumption and higher utility loss compared to both Ricardian 1 and Control (see column *Total* in Table 2).

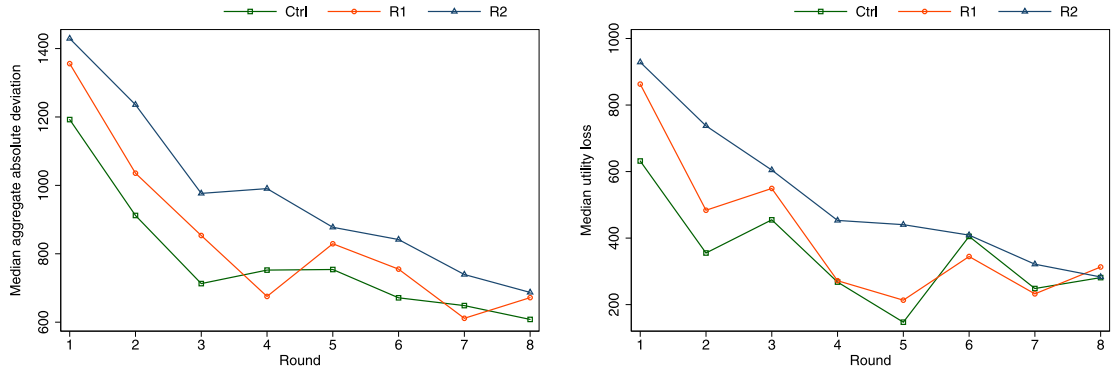
Examining the differences across treatments in the specific rounds reveals that this relationship is significant for many, but not all rounds. Absolute differences from optimal consumption (measure m_1) are significantly higher in Ricardian 2 compared to Control in six of eight rounds. When comparing m_1 between Ricardian 1 and Control, m_1 is significantly higher in Ricardian 1 compared to Control in three out of eight rounds. Comparing m_1 between Ricardian 1 and Ricardian 2 reveals

¹⁵ This is a consequence of our final period condition given in (3).

¹⁶ Note that the intertemporal budget constraint implies that total consumption is the same for each subject in a given round and depends only on the realization of net income plus initial endowment, i.e. $\sum_{t=1}^T c_t = \sum_{t=1}^T y_t - \sum_{t=1}^T \tau_t + a_1$.

¹⁷ Recall that this is not true for subjects in the treatment Ricardian 3. For this reason, the analysis in this section excludes observations from that treatment.

¹⁸ Additionally, the Online Appendix contains Fig. C.1, in which welfare loss is denominated in terms of money that subjects loose by behaving suboptimally.



(a) Aggregate Deviations from Optimal Consumption (m_1)

(b) Aggregate Utility Loss (m_2)

Fig. 1. Deviations from optimal consumption and utility loss.

Table 2
Medians of the measures m_1 and m_2 by treatments and rounds.

	Total	Round							
		1	2	3	4	5	6	7	8
Median									
m1 Ctrl	759.75	1192.37	912.01	712.87	752.35	754.05	671.31	648.53	608.10
m1 R1	838.66	1355.96	1035.61	853.42	675.27	829.30	755.12	611.36	671.83
m1 R2	949.25	1429.39	1235.89	976.59	990.56	877.47	841.66	739.42	687.04
p-Value									
R1-Ctrl	0.02	0.03	0.51	0.24	0.10	0.43	0.37	0.00	0.01
R2-Ctrl	0.00	0.00	0.00	0.00	0.08	0.03	0.32	0.30	0.08
R1-R2	0.00	0.09	0.00	0.00	0.01	0.01	0.00	0.00	0.66
Median									
m2 Ctrl	350.35	631.76	355.19	454.94	267.78	147.19	405.13	248.38	281.20
m2 R1	381.70	862.93	483.72	549.26	271.92	213.33	344.64	232.34	313.13
m2 R2	497.61	928.82	737.49	604.52	453.11	440.39	408.98	321.48	283.18
p-Value									
R1-Ctrl	0.00	0.00	0.46	0.12	0.01	0.14	0.06	0.00	0.00
R2-Ctrl	0.00	0.00	0.00	0.00	0.74	0.00	0.68	0.04	0.04
R1-R2	0.00	0.01	0.00	0.00	0.01	0.00	0.00	0.00	0.17

Notes: P-values of Mann-Whitney U-tests.

that absolute deviations from optimal consumption are significantly higher in Ricardian2 in all but two rounds. Overall this finding confirms the above intuition, though the evidence is not very strong in comparing Ricardian1 and Control at the round level. In Table C.1 in the Online Appendix we replicate these results using regression techniques.¹⁹

Comparing measure m_2 (utility loss) at the round level across the three different treatments yields similar results. Utility loss is significantly higher in Ricardian2 than in Control in all but rounds four and six. Measure 2 is significantly higher in Ricardian1 compared to Control in four out of eight rounds. With respect to the Ricardian treatments, utility loss in Ricardian2 is significantly higher than in Ricardian1 in seven of the eight rounds.

Deviations from optimal consumption as well as utility loss appear to decline over the eight rounds of the experiment. This supports the idea that subjects learn to improve their consumption decisions by repeating the experiment. We investigate learning behaviour in more detail in Section 3.3.

In summary, subjects in treatments with Ricardian taxation have higher deviations from optimal consumption and a higher utility loss than subjects in the Control treatment. Moreover, subjects with a net income stream that is difficult to smooth (Ricardian2) appear to perform worse than subjects with a net income stream that is easy to smooth. These findings imply that subjects react to both difficulty to smooth consumption and Ricardian taxation. However, the finding that subjects in Ricardian1 appear to perform worse than subjects in the Control treatment suggests that the effect of Ricardian taxation

¹⁹ We report regression results using treatment dummies $dR1$ and $dR2$, round dummies $d_{r,1}$ to $d_{r,8}$, and their interactions as regressors and aggregate absolute deviations m_1 as dependent variable. This regression reproduces the results from the nonparametric analysis in Table 2. The constant is statistically not different from the corresponding median value of the Control treatment for round 1 in Table 2. The coefficients on the treatment dummies are similarly not different from the respective median value for round 1. The interaction terms show how these medians change over rounds and treatments.

outweighs that of the decreased difficulty to smooth consumption. One mechanism that would result in such a finding is that subjects do not internalise the government budget constraint but instead treat a tax benefit as additional wealth.

3.2. Panel regression

In order to assess the magnitude of the effect that Ricardian taxation has on consumption, we run structural panel regressions.

Our baseline specification derived from Eq. (5) is

$$c_{itr} = \beta_1 \tilde{y}_{tr} + \beta_2 \tilde{a}_{itr} + \beta_3 (T - t) \tilde{y}_p - \beta_4 \tilde{\tau}_{itr} - \beta_5 \tilde{\Gamma}_{tr}(\theta, \sigma_y), \quad (10)$$

for all subjects $i = 1, \dots, 176$, periods $t = 1, \dots, 25$, and rounds $r = 1, \dots, 8$ where $\tilde{F} = \frac{1}{(T-t+1)}F$, and F represents the variables of Eq. (5).²⁰ We transform the regressors that are derived from the theoretical consumption function in this way to account for the time dependency of optimal consumption. Moreover, this simplifies the interpretation of the corresponding coefficients. If subjects behave optimally, or deviate randomly from optimal consumption, e.g. due to calculation errors, the estimated coefficients β_1 to β_5 should be equal to one.

Hypothesis 1. β_1 to β_5 are not statistically different from one.

If we reject this hypothesis, subjects deviate systematically from optimal consumption. In Eq. (11), we extend our baseline specification to account for tax effects by including dummy variables indicating a tax rebate $d_{0,tx}$ and a tax increase $d_{240,tx}$. Moreover, we control for treatment using treatment dummies ($dR1$, $dR2$, $dR3$) and subject characteristics X_i such as risk preference, gender, and subject of academic study.²¹ Finally, we account for round effects and include a constant, period, and period squared. The latter two variables should capture any time trend that is beyond the theoretical.

$$\begin{aligned} c_{itr} = & \beta_1 \tilde{y}_{tr} + \beta_2 \tilde{a}_{itr} + \beta_3 (T - t) \tilde{y}_p - \beta_4 \tilde{\tau}_{itr} - \beta_5 \tilde{\Gamma}_{tr}(\theta, \sigma_y) \\ & + \beta_{0,tx} d_{0,tx} + \beta_{240,tx} d_{240,tx} + \beta_6 dR1_i + \beta_7 dR2_i + \beta_8 dR3_i \\ & + \beta_9 X_i + \sum_{k=1}^8 \beta_{r,k} d_{r,k} + \beta_{10} t + \beta_{11} t^2 + \text{constant}. \end{aligned} \quad (11)$$

Since all these additional regressors do not show up as variables in the optimal consumption function, the corresponding coefficients should not be significantly different from zero if subjects behave optimally or deviate randomly from optimal consumption.

Hypothesis 2. $\beta_{0,tx}$ and $\beta_{240,tx}$, $\beta_{r,1}$ to $\beta_{r,8}$, and β_6 to β_{11} are not statistically different from zero.

If we reject this hypothesis, subjects deviate systematically from optimal consumption. Table 3 shows what factors are associated with observed consumption (c_{itr}).²² The first specification reported in this table is the baseline model (Eq. (10)) plus a constant which should be equal to zero according to the theory. The second specification refers to the extend model in Eq. (11). Individual specific characteristics, such as ability to use computer software, could bias our estimates. To obtain consistent results, we estimate a fixed effects (FE) specification that is presented along with the OLS specifications. All specifications are estimated with robust standard errors clustered at the subject level. In the following analysis we will focus on the more robust FE estimation, while acknowledging any substantial differences in the different specifications. Recall that if subjects behave optimally or deviate randomly from optimal consumption, the estimated coefficients β_1 to β_5 should equal one.

For β_1 , the data reject this hypothesis. Table 3 shows that the coefficient for current income is significantly higher than one. This implies that individuals react to changes in current income more strongly than optimal. While this finding conflicts with the theory, it is consistent with the notion of excess sensitivity from the empirical literature.²³ Subjects consistently do not only consume too much out of current income, but also out of expected income. The estimate for the coefficient on $(T - t) \tilde{y}_p$ is of similar size, however, only statistically different from one in the baseline specification. Subjects do not seem to have correct intuition about what the levels of current and expected income imply for their decision problem, or they simply overreact to income changes.

The coefficient on savings indicates that subjects do not spend enough out of wealth since the estimate is statistically smaller than one throughout. This could again stem from difficulties in assessing magnitudes, or it could reflect a social norm that deems parsimony as a virtue.

The amount of future due taxes might not have been assessed correctly either. The coefficient in the extended specifications is about one third of what theory predicts, while the estimate in the baseline specification is quite imprecise with

²⁰ The total of 176 subjects includes 44 subjects in treatment Ricardian 1, 47 in Ricardian 2, 43 in Ricardian 3 and 42 in Control.

²¹ Subjects who are not students, i.e. unemployed or employees, are subsumed under *other* in Table 3.

²² We suppress henceforth subject and round indices to facilitate legibility.

²³ See e.g. Flavin (1981), Hall and Mishkin (1982), Souleles (1999), Shea (1995) and Parker (1999). Several explanations for excess sensitivity are debated in the literature; in particular, myopic behaviour, liquidity constraints, and buffer-stock saving.

Table 3
Panel regressions on observed consumption.

	Baseline OLS		Extended OLS		FE	
\bar{y}	1.135**	(17.77)	1.160**	(18.17)	1.187***	(22.56)
\bar{a}	0.642***	(196.36)	0.655***	(87.40)	0.780***	(29.06)
$(T - t)\bar{y}_p$	1.165*	(13.78)	1.130	(7.32)	1.204	(6.92)
$\bar{\mathcal{F}}$	1.065	(4.20)	0.272***	(4.47)	0.307***	(4.49)
$\tilde{\Gamma}(\theta, \sigma_y)$	3.318	(1.76)	2.017	(0.58)	2.180	(0.63)
Tax dummies (base: 120):						
d_0.tx			23.50***	(6.05)	25.26***	(5.44)
d_240.tx			-28.71***	(-7.87)	-30.02***	(-6.69)
t			-3.038***	(-3.26)	-3.037***	(-3.25)
t ²			0.101	(1.57)	0.104	(1.64)
Treatment (base: Control):						
dR1			-25.16*	(-1.89)		
dR2			-21.41*	(-1.96)		
dR3			-33.92**	(-2.16)		
Round dummies (base: round 1):						
d _{r,2}			-5.153	(-0.94)	-6.389	(-1.00)
d _{r,3}			-4.073	(-0.76)	-3.586	(-0.56)
d _{r,4}			-1.573	(-0.27)	-2.544	(-0.38)
d _{r,5}			-0.992	(-0.18)	-1.224	(-0.19)
d _{r,6}			-4.587	(-0.82)	-3.569	(-0.53)
d _{r,7}			-3.283	(-0.59)	-2.832	(-0.43)
d _{r,8}			-3.489	(-0.63)	-2.868	(-0.43)
Risk aversion (base: low):						
high			-23.92*	(-1.82)		
medium			-16.07	(-1.36)		
Gender (base: male):						
female			24.80*	(1.95)		
Subject (base: economics):						
engineering			-4.834	(-0.58)		
otherscience			1.692	(0.20)		
other			-22.96	(-1.56)		
Constant	20.64	(0.81)	-40.32	(-1.42)	-85.14***	(-5.37)
Adjusted R ²	0.498		0.509		0.603	
Overall R ²					0.498	

Notes: The dependent variable is observed consumption (c_{it}). T-statistics based on cluster robust (subject level) standard errors are in parentheses. T-statistics and significance levels of the first five regressors refer to tests of the H_1 (see text) that the respective variable is equal to 1, significance levels are * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All other t statistics and significance levels refer to tests of the H_2 (see text) for which the respective variable is equal to zero; significance levels are * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

a 95% confidence interval that includes both 0.57 and 1.57. A ceteris paribus interpretation of the much more precise FE estimate implies that one Taler less (the variable is defined as -1 times the original variable) of future taxes to be paid increases spending by 0.307 Taler instead of one.

The impact of precautionary saving on consumption should be captured by the coefficient on $\tilde{\Gamma}(\theta, \sigma_y)$. The estimated coefficient is approximately twice as high as theory would predict in the extended specification, but not statistically different from one in both extended and the baseline specifications.²⁴ Note also, that the coefficient on the period (t) is significantly negative. The regressors t and t^2 were included to capture any time trend beyond the theoretical. The negative coefficient on t implies that subjects consume too much in early periods compared to later periods. This is consistent with other intertemporal consumption experiments, where overconsumption is regularly found.²⁵

The coefficients of our particular interest are $\beta_{0,tx}$ and $\beta_{240,tx}$ because they indicate how subjects react to a tax rebate ($\tau_t = 0$) and a tax increase ($\tau_t = 240$) respectively.

In the FE specification, the estimated coefficient $\beta_{0,tx}$ is 25.26 (p-value: < 0.01). This implies that a tax rebate of 120 Taler is associated with an increase in consumption of 25.26 Taler, or 21% of the tax rebate. In turn, the estimated coefficient corresponding to a tax increase ($\beta_{240,tx}$) is -30.02 (p-value: < 0.01), implying that an increase in taxes of 120 Taler is associated with a decrease in consumption of 30.02 Taler, or about 25% of the tax increase.

These results give account of the average effect of taxation in all Ricardian treatments. However, we are also interested in whether reactions to taxation differ by treatment. We can identify the effects of Ricardian taxation separately by including interaction terms of $d_{0,tx}$ and $d_{240,tx}$, with binary variables indicating treatment Ricardian 1, Ricardian 2 and Ricardian 3 respectively.

²⁴ See, e.g., Fossen and Rostam-Afschar (2013) who show that it is difficult to measure the importance of precautionary saving and to disentangle it from entrepreneurial saving with survey data.

²⁵ See Duffy (2012) for an excellent survey on intertemporal consumption experiments.

Table 4
Change of deviations from optimal consumption over rounds.

	Round						
	2	3	4	5	6	7	8
Median $m_1^1 - m_1^r$	147.09	262.61	244.56	294.00	371.32	355.30	415.85
p-value	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Median $m_1^{r-1} - m_1^r$	147.09	81.96	31.17	48.32	17.46	42.66	23.10
p-value	0.00	0.00	0.18	0.06	0.14	0.02	0.26

Notes: Median differences in m_1 between consecutive rounds and to the first round. P-Values of Wilcoxon matched-pairs signed-ranks test in parentheses. The null hypothesis is that both distributions are the same.

In treatment Ricardian 1, the estimated coefficient corresponding to a tax rebate is 15.52 (p-value = 0.017) and the coefficient corresponding to a tax increase is -36.2 (p-value < 0.01).²⁶ In treatment Ricardian 2 the coefficient corresponding to a tax rebate is 27.14 (p-value < 0.01) and that corresponding to a tax increase is -20.24 (p-value < 0.01).²⁶ Finally, in treatment Ricardian 3 the coefficient corresponding to a tax rebate is 27.03 (p-value < 0.01) and that corresponding to a tax increase is -29.45 (p-value < 0.01).²⁶ These estimates indicate that subjects react to taxes in a similar way in all treatments. The coefficients associated with a tax rebate turn out *not* to be significantly different in the treatments Ricardian 1, Ricardian 2, and Ricardian 3 in mutual and joint tests. A significant difference (mutual p-value = 0.03, joint p-value = 0.09) is observed only between the coefficients corresponding to a tax increase of Ricardian 1 and Ricardian 2.²⁷

Overall, our results suggests that taxes have a significant and strong effect on consumption. This is in stark contrast with the theoretical predictions, and thus we conclude that the Ricardian proposition is rejected by the experimental data. An early tax benefit causes a significant increase in consumption on average. The corresponding later increase in taxation causes a significant decrease in consumption on average.

Our findings are based on pooling all subjects. However, there appears to be some heterogeneity in our experimental data that cannot be controlled for, even with a fixed effects specification. Generally, this occurs when subjects employ different strategies to choose consumption. Therefore, we run individual OLS regressions for each subject, using the same specification as above to identify the share of subjects that behaves in accordance with Ricardian Equivalence. This allows to estimate different coefficients for each subject and thus accounts for different strategies.²⁸ We classify the subjects' behaviour as follows: if either the coefficient associated with a tax benefit ($\beta_{0,tx}$), the coefficient associated with a tax increase ($\beta_{240,tx}$), or both are significantly different from zero at the 5% level, a subject's behaviour is inconsistent with Ricardian Equivalence. In this conservative way, we find that the behaviour of approximately 56% of our subjects in the Ricardian treatments can be classified as being not consistent with Ricardian Equivalence. If we only require the coefficient associated with a tax benefit ($\beta_{0,tx}$) not to be statistically different from zero at the 5% level, about 34% of our subjects are classified as being not consistent with Ricardian Equivalence.

3.3. Learning

It has been shown in many experiments on intertemporal optimisation that subjects tend to improve their decision-making towards optimality when playing the experiment repeatedly.²⁹ This is typically interpreted as evidence for learning. Fig. 1 of Section 3.1 suggest that this may also be the case in our experiment.

In order to formally test whether subjects improve their consumption decisions, we analyse within subject differences of absolute deviations from optimal consumption (Measure m_1) across rounds. Table 4 contains the median of within subject differences in absolute deviations from optimal consumption between round r and the first round ($m_1^1 - m_1^r$), as well as the median difference in absolute deviations from optimal consumption between two consecutive rounds ($m_1^{r-1} - m_1^r$). There is a significant reduction in deviations from optimal consumption between rounds one and two, two and three, six and seven, and a marginally significant reduction between rounds four and five (p-value = 0.06). However, all rounds show significant improvement of consumption decisions in comparison to the first round. Hence subjects seem to be able to improve their consumption decisions with repetition of the experiment.

The main goal of this paper is to test if subjects learn to behave according to the prediction of the Ricardian proposition. That is whether they learn *not* to react to tax cuts and tax increases with their consumption choices. To answer this question, we ran two additional specifications of the panel regressions introduced in Section 3.2. In specification FE (1) in Table 5 we include interaction terms of the tax dummy for a tax cut and a tax increase with a dummy indicating each round,

²⁶ Not reported in Table 3.

²⁷ Table C.2 in the Online Appendix shows the FE regressions for the Control treatment and each Ricardian treatment in comparison to the Control treatment. The coefficients for tax rebates and increases show a similar pattern though they change somewhat due to smaller sample size.

²⁸ Here we abstract from learning effects. We investigate the different behavioural strategies with a focus on learning in Section 3.4.

²⁹ See for instance Ballinger et al. (2003), Carbone and Hey (2004), Brown et al. (2009), Meissner (2016).

Table 5
Learning effects in panel regression on observed consumption.

	FE (1)		FE (2)	
\bar{y}	1.185***	(22.66)	1.192***	(22.49)
\bar{a}	0.781***	(29.10)	0.780***	(29.13)
$(T - t)\bar{y}_p$	1.203	(6.97)	1.194	(6.93)
$\bar{\mathcal{F}}$	0.296***	(4.34)	0.438***	(6.45)
$\bar{\Gamma}(\theta, \sigma_y)$	2.201	(0.64)	2.114	(0.61)
Tax dummies (base: 120):				
$d_{0,tX}$	30.85***	(3.42)		
$d_{240,tX}$	-36.75***	(-4.23)		
Interaction of tax cut and round (base: round 1, 120):				
$d_{r,2} \times d_{0,tX}$	0.406	(0.04)	28.98***	(4.97)
$d_{r,3} \times d_{0,tX}$	-1.541	(-0.16)	26.69***	(4.27)
$d_{r,4} \times d_{0,tX}$	-4.345	(-0.45)	24.16***	(3.84)
$d_{r,5} \times d_{0,tX}$	-8.157	(-0.89)	20.42***	(3.93)
$d_{r,6} \times d_{0,tX}$	-9.872	(-1.05)	18.68***	(3.41)
$d_{r,7} \times d_{0,tX}$	-9.551	(-1.04)	19.04***	(3.51)
$d_{r,8} \times d_{0,tX}$	-11.23	(-1.20)	17.16***	(3.20)
Interaction of tax increase and round (base: round 1, 120):				
$d_{r,2} \times d_{240,tX}$	-0.512	(-0.06)	-33.86***	(-6.64)
$d_{r,3} \times d_{240,tX}$	5.716	(0.64)	-25.27***	(-5.45)
$d_{r,4} \times d_{240,tX}$	4.317	(0.46)	-28.79***	(-5.01)
$d_{r,5} \times d_{240,tX}$	4.830	(0.52)	-28.46***	(-5.07)
$d_{r,6} \times d_{240,tX}$	11.72	(1.29)	-21.62***	(-4.28)
$d_{r,7} \times d_{240,tX}$	9.182	(1.07)	-24.13***	(-5.24)
$d_{r,8} \times d_{240,tX}$	16.76*	(1.90)	-16.19***	(-3.15)
Round dummies (base: round 1):				
$d_{r,2}$	-6.345	(-0.87)	-6.329	(-0.97)
$d_{r,3}$	-3.977	(-0.54)	-3.625	(-0.55)
$d_{r,4}$	-2.515	(-0.33)	-2.450	(-0.35)
$d_{r,5}$	-0.881	(-0.12)	-0.875	(-0.13)
$d_{r,6}$	-3.704	(-0.49)	-3.673	(-0.54)
$d_{r,7}$	-2.762	(-0.37)	-2.781	(-0.41)
$d_{r,8}$	-3.359	(-0.45)	-3.171	(-0.47)
t	-3.088***	(-3.30)	-2.738***	(-2.88)
t^2	0.106*	(1.67)	0.0852	(1.32)
Constant	-85.99***	(-5.37)	-66.25***	(-4.19)
Adjusted R^2	0.603		0.602	
Overall R^2	0.498		0.498	

Notes: The dependent variable is observed consumption (c_{itr}). T-statistics based on cluster robust (subject level) standard errors are in parentheses. T-statistics and significance levels of the first five regressors refer to tests of the H_0 that the respective variable is equal to 1, significance levels are * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All other t statistics and significance levels refer to tests of the H_0 for which the respective variable is equal to zero; significance levels are * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

respectively. This allows to see whether there is a reduction in the reaction to a tax cut or tax increase, relative to the baseline.

Hypothesis 3. $\beta_{0,tX \times r2}$ to $\beta_{0,tX \times r8}$, $\beta_{240,tX \times r2}$ to $\beta_{240,tX \times r8}$, $\beta_{0,tX}$, and $\beta_{240,tX}$ are not statistically different from zero.

If we reject one of these hypotheses, consumption choices are not made in accordance with Ricardian Equivalence. The partial effect of a tax cut (increase) on consumption in round 1 (when all round dummies are zero) is significantly positive (negative). Significantly negative (positive) interaction terms imply that a tax cut (increase) yields a lower effect on consumption in the particular round. Only the coefficient of a tax increase interacted with round eight is significant at the 10% level, while the absolute magnitude of the coefficients increases with round.

Still, reductions in the reaction to tax cuts and tax increases do not imply that reactions disappear entirely. This is clearer in specification FE (2) in which we exclude as regressors the dummies for tax cuts and tax increases, respectively. The coefficients of the interactions of the dummies for a tax-cut and a tax-increase with round dummies may then be interpreted as the absolute effect taxation has on consumption in each particular round.

The absolute values of the coefficients on these interactions are decreasing over rounds. However, all coefficients are highly significantly different from zero. This implies that taxation affects consumption in all eight rounds.

Summing up, we observe that subjects generally improve their consumption decisions towards optimality when repeating the experiment. We find some evidence that the extent to which subjects react to tax increases decreases with rounds. However, even after eight rounds of learning, tax cuts and increases have a significant impact (both statistically and

Table 6
Predictions for Ricardian Equivalence.

Behavioural model	Equation	Ricardian Equivalence
Optimal Consumption	(5)	holds
σ -Zero-Approximation	(12)	holds
Constant Consumption	(13)	holds
Fraction of Net Income	(14)	does not hold
Fraction of Cash on Hand	(15)	does not hold

economically) on consumption choices. This implies that subjects on average do not learn to comply with the Ricardian Equivalence in the eight repetitions of our experiment.

3.4. Rules of thumb

Even though our experimental environment is highly stylised, finding the optimal solution is a nontrivial task. A natural starting point to model behaviour and analyse learning is therefore the idea that subjects use rules of thumb (e.g. [Cochrane, 1989](#)). In this section, we investigate the usage of the following rules of thumb:

1. σ -zero-approximation

According to this rule, subjects consume as if income uncertainty were zero ($\sigma_y = 0$). That is, the optimal decision rule is approximated by abstracting from precautionary saving. This simplifies [Eq. \(5\)](#) to

$$c_t^{\sigma=0}(w_t) = \frac{1}{T-t+1} [w_t + (T-t)y_p - \mathcal{T}_t]. \quad (12)$$

This rule describes behaviour of subjects that are forward looking, i.e. subjects take the number of remaining periods into account, calculate permanent income and the amount of taxes to be paid. Effectively, they calculate consumption as the expected funds available to them in the remaining periods, divided by the number of periods remaining. Since aggregate income uncertainty is relatively small in this experiment, precautionary saving is also relatively small. Therefore, this consumption rule is a rather close approximation of optimal consumption. Moreover, since all tax payments are taken into consideration correctly, agents who follow this rule should not violate Ricardian Equivalence.

2. Constant consumption

Subjects who behave according to this rule are forward looking because they calculate their (ex-ante) expected net resources and smooth this value over time. [Eq. \(12\)](#) is approximated with information on the initial and final period only by

$$c_t^{const} = c_{t+1} = \dots = c_{T-1} = \frac{1}{T} (a_1 - \vartheta) + y_p = 105. \quad (13)$$

Subjects with this consumption function may be quite far from the optimal solution but it is important to note that this decision rule implies Ricardian Equivalence.

3. Constant fraction of period net income

Another potential rule of thumb is consuming a constant fraction of period net income. In our experimental environment, this alone would be a poor consumption rule due to the initial endowment. Therefore, in the rule of thumb specified below, agents smooth their initial endowment over the life-cycle, and consume a constant fraction of their net income on top in each period.

$$c_t^y = \frac{1}{T} a_1 + \kappa_y [y_t - \tau_t]. \quad (14)$$

Agents who behave according to this rule violate Ricardian Equivalence.

4. Constant fraction of period cash on hand

With this rule, agents consume a constant fraction of their wealth minus taxes (= cash on hand) in each period. This rule is modelled after the “Keynesian” consumption function and is similar to one of the rules considered in [Brown et al. \(2009\)](#). Agents who behave according to this rule violate Ricardian Equivalence.

$$c_t^w(w_t) = \kappa_w [w_t - \tau_t]. \quad (15)$$

In addition, we also consider optimal consumption, as specified in [Eq. \(5\)](#), as one potential consumption rule. For convenience, we report the theoretic predictions regarding Ricardian Equivalence of each considered consumption rule in [Table 6](#).

To match behaviour of our subjects to one of the above consumption rules, we calculate consumption levels predicted by each of the consumption rules. We calculate κ_y and κ_w using Monte Carlo simulations such that the parameters yield rule specific optimal consumption. In this way, the degrees of freedom identical across all rules. We then calculate the root mean squared error (RMSE) of observed consumption and the prediction of each consumption rule for each subject and each

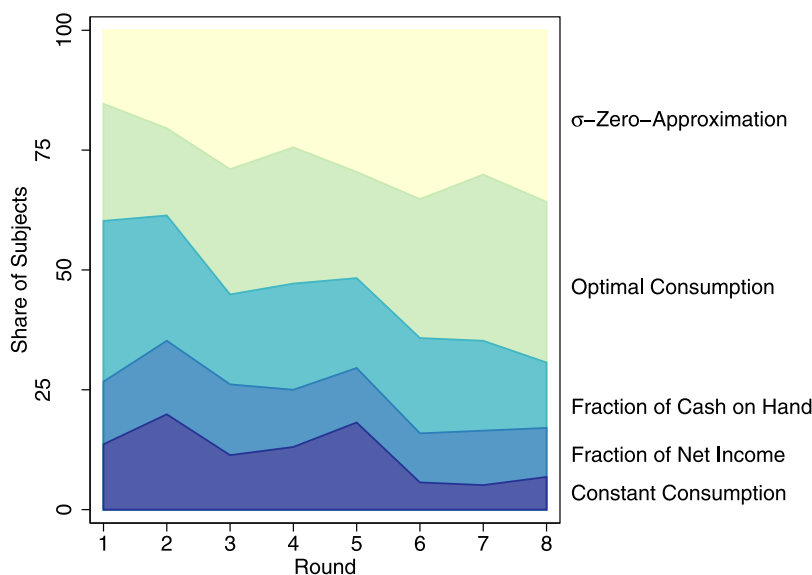


Fig. 2. Rules of thumb.

round. The smaller the RMSE, the better a consumption rule describes behaviour. We therefore classify subjects to each of these rules in each round if the respective RMSE is smallest.³⁰

Fig. 2 shows the resulting composition of our sample. The figure displays the percentage of our sample that uses each of the rules, sorted by frequencies in round 8, where most of the subjects should have finished the learning process. If no subject had changed the strategy over rounds, the figure would show 5 areas separated by 4 horizontal lines. This is clearly not the case.

In round one, only about 40% of our subjects reveal behaviour that can be best described by optimal consumption or the very close to optimal σ -zero-approximation. 14% of the subjects appear to follow the constant consumption strategy in this round. Subjects' behaviour in the first round is most frequently best described by the strategy to consume a constant fraction of wealth (34%). The share of subject whose behaviour is best described by consuming a fraction of net income remains relatively stable over the eight rounds at around 10–15%.

The main result with regard to learning behaviour is that, over the course of the eight rounds, the share of subjects who appear to consume optimally or near optimally increases drastically to about 70%. This striking increase is contrasted by the share of subjects who consume a constant amount or a constant fraction of their wealth. Their share decreases by roughly the same magnitude, to about 20%. This implies that subjects do learn to improve their consumption decisions, by changing their consumption rules over the course of the experiment.

What does this imply for Ricardian Equivalence? In the first round, about 47% follow strategies that violate Ricardian Equivalence (*fraction of net income*, *fraction of cash on hand*). In round 8, this fraction is halved; only 24% follow these strategies. This suggests that some subjects learn Ricardian Equivalence, by adjusting their employed consumption rules. This can be confirmed by Table 7, which provides for each round the percentage of subjects who appear to switch their strategy.

The last row in Table 7 reports the total fraction of subjects who appear to switch their strategy over all rounds of the experiment. In the early rounds, the percentage of people who switch strategies is relatively high: about 57% of all subjects choose a different strategy in the second round compared to the first round. This fraction declines over rounds to about 36% in the last round. Considering the individual consumption rules, the striking pattern is that subjects switch in the second round to each of the strategies with roughly the same probability. This changes over rounds. In particular, increasingly more subjects switch to the σ -zero-approximation or the optimal rule. Transitions to rules that violate Ricardian Equivalence decline substantially towards the later rounds.³¹

An interesting question is now whether the consumption rule that people employ has predictive power regarding whether subjects comply with Ricardian Equivalence or not.

³⁰ See e.g. Ballinger et al. (2003), who use the same method to categorise subjects according to different apparent planning horizons. See Fig. C.3 in the Online Appendix for the median RMSE separated by types and rounds.

³¹ While Table 7 illustrates what rules subjects switch to between rounds, we also report the reverse, i.e. from what rules subject switch to other rules, in the Online Appendix: Table C.4 shows that transition to rules that are in accordance with Ricardian Equivalence are mostly higher than transition from these rules. In contrast, for the non-Ricardian rules efflux is almost always greater than influx.

Table 7
Strategy evolution.

Strategy	Unit	Round						
		2	3	4	5	6	7	8
to σ -Zero-Approximation	(%)	14	16	14	14	20	10	16
to Optimal Consumption	(%)	11	19	18	10	17	14	10
to Constant Consumption	(%)	14	9	10	9	5	3	5
to Fraction of Cash on Hand	(%)	9	5	8	4	7	6	2
to Fraction of Net Income	(%)	9	7	3	5	5	5	2
Total Switchers	(%)	57	56	53	41	53	37	36

Table 8
Reaction to tax changes across heuristics over rounds.

	Round							
	1	2	3	4	5	6	7	8
Optimal Consumption								
$d_{0,t,x}$	13.37* (1.81)	12.33** (2.42)	10.19** (2.28)	11.88*** (3.11)	8.424* (1.70)	13.95*** (2.69)	8.474*** (2.83)	8.733** (2.34)
$d_{240,t,x}$	-31.10*** (-3.44)	-7.080 (-1.07)	-17.87*** (-2.88)	-15.07*** (-3.91)	-19.72** (-2.16)	-21.03*** (-4.78)	-12.57*** (-2.77)	-4.773 (-1.04)
σ-Zero-Approximation								
$d_{0,t,x}$	14.34 (1.18)	6.884 (0.90)	12.24** (2.20)	13.77*** (3.19)	13.58*** (2.80)	4.535 (1.60)	5.314 (1.25)	3.375 (0.64)
$d_{240,t,x}$	-11.52 (-1.20)	-12.93* (-1.87)	-12.99*** (-2.84)	-17.71*** (-3.34)	-19.18*** (-3.14)	-12.42*** (-3.06)	-9.680** (-2.23)	-8.155** (-2.37)
Constant Consumption								
$d_{0,t,x}$	13.32* (2.02)	-0.0964 (-0.02)	18.01 (1.54)	-2.151 (-0.61)	5.394 (0.88)	10.31 (0.98)	-4.440 (-0.81)	1.631 (0.26)
$d_{240,t,x}$	-9.915 (-1.10)	-7.676 (-1.34)	-23.63** (-2.47)	-7.167 (-0.83)	-6.472 (-0.80)	2.780 (0.27)	-31.17 (-1.15)	-14.21 (-1.44)
Fraction of Net Income								
$d_{0,t,x}$	140.0*** (5.15)	74.62*** (3.65)	95.70*** (4.56)	35.48* (1.92)	71.35*** (4.28)	76.45*** (5.29)	85.36*** (7.31)	81.44*** (5.02)
$d_{240,t,x}$	-69.99*** (-6.79)	-81.77*** (-6.16)	-59.95*** (-5.15)	-77.48*** (-4.61)	-83.91*** (-7.94)	-54.33*** (-5.35)	-58.97*** (-4.63)	-63.59*** (-7.92)
Fraction of Cash on Hand								
$d_{0,t,x}$	41.82*** (3.95)	18.56** (2.26)	9.625 (0.50)	49.32** (2.23)	46.79*** (2.97)	21.24 (1.56)	39.87** (2.24)	35.25** (2.40)
$d_{240,t,x}$	-35.51** (-2.20)	-14.04 (-1.10)	-34.36*** (-3.99)	-40.40*** (-2.86)	-55.03*** (-5.53)	-42.84*** (-3.66)	-47.54*** (-3.46)	-35.93*** (-3.37)
Additional Controls	✓	✓	✓	✓	✓	✓	✓	✓

Notes: The dependent variable is observed consumption (c_{it}). T-statistics based on cluster robust (subject level) standard errors are in parentheses. T-statistics and significance levels refer to tests of the H_0 for which the respective variable is equal to zero; significance levels are * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

To test this, we divide our sample in each round by the five strategies. We then run our extended specification (Eq. (11)) individually for each strategy and round, such that the estimates show the joint effect of learning over time of subjects who stick to a strategy and of those who switch strategies. Therefore, the number of observations varies by round and strategy, as illustrated in Fig. 2.³²

Since we are interested in learning Ricardian behaviour, we only report the estimated coefficients on the tax cut and the tax increase dummies over different rounds for each strategy.³³

The differences in rules reported in Table 8 are as expected: the strategies that are consistent with Ricardian Equivalence have much smaller coefficients on the dummies for tax cuts and increases compared to the strategies that are predicted to violate Ricardian Equivalence.

Moreover, Table 8 provides another piece of evidence that some subjects learn Ricardian Equivalence. Even though the theoretical prediction for the rules *optimal consumption* and *σ -zero-approximation* is that Ricardian Equivalence should hold, most point estimates are significantly positive (negative) for a tax cut (increase). Over the eight rounds, the magnitudes of the point estimates appear to grow somewhat smaller, though only in few cases significantly. This suggests that subjects learn to react less to tax cuts and increases, but still show small reactions on average.

³² Note that this split decreases the sample size for each of the individual estimations drastically. Therefore, the results should be interpreted with caution.

³³ Full estimation results are available on request.

Strikingly, for the constant consumption rule we cannot find any significant effect of taxes in all rounds, except for the tax increase in round 3 and the tax cut dummy in round 1 which are on the 5% and 10% level significantly different from zero, respectively. This is the only rule where tax increases almost never influence consumption significantly. Subjects who use this strategy can therefore be classified as Ricardians throughout.

Turning to the strategies that are predicted to violate Ricardian Equivalence, an entirely different pattern emerges. Again, most of the point estimates on tax cuts and increases are significant. However, the coefficients on tax cuts and increases are much larger than for the other strategies. Moreover, the magnitudes of the coefficients do not appear to decline over the eight rounds.

For example, the tax cut estimate for the strategy to consume a fraction of net income is even larger in round 8 compared to round 2, though this difference is not statistically significant. Similarly, tax increases lead to reactions of comparable magnitude in rounds 1 and 8. This is also true for the strategy to consume a constant fraction of cash on hand. This implies that subjects who employ these rules do not behave according to Ricardian Equivalence, and they also do not react less to tax cuts and increases in the course of the eight rounds.³⁴

Summing up, reactions to tax cuts and increases are much higher for subjects who appear to consume a *constant fraction of cash on hand* or *net income*, compared to subjects who appear to consume *optimally* or *near optimally*. People who appear to consume a *constant amount* show the least reactions to tax cuts and increases.

4. Concluding remarks and discussion

In this paper we test whether subjects consume according to Ricardian Equivalence, and whether they learn to comply with the Ricardian Equivalence proposition in a life cycle consumption laboratory experiment.

We find that changes in taxation have significant and strong average effects on consumption in our sample. A tax benefit in early periods increases consumption by about 21% of the tax benefit on average, while a tax increase causes a reduction by 25% of the tax increase.

At the individual level, we find that the behaviour of a significant portion of our subjects can be classified as inconsistent with the Ricardian Equivalence proposition. A conservative estimation suggests that this portion is about 56%. This finding uses evidence on both tax cuts and increases. In survey or register data, often only a tax cut is observed for any one individual. If we test for compliance with Ricardian Equivalence based only on tax cuts, about 34% of our subjects are classified as being not consistent with Ricardian Equivalence.

This finding is similar to those in other studies that employ very different methods. For instance, [Campbell and Mankiw \(1991\)](#) use aggregate data to find the fraction of consumers who respond to changes in current disposable income to be in the range of 35%–50% for the United States and lower fractions in other countries. [Shapiro and Slemrod \(1995\)](#) find from a telephone survey that 43% of those who responded said they would spend most of the extra take-home pay.

Given the large share of our subjects who violate Ricardian Equivalence, we investigate whether subjects learn Ricardian Equivalence. We find that subjects on average do not appear to learn to consume in accordance with the Ricardian Equivalence proposition. Although subjects in our experiment learn to improve their consumption decisions, their reaction to tax cuts and tax increases remain significant, even after eight repetitions of the experiment.

In a next step, we show that subjects appear to use different rules of thumbs to approximate optimal consumption, in the spirit of studies like [Mankiw \(2000\)](#) or [Galí et al. \(2004\)](#) that explicitly model two types of consumers (e.g. rule-of-thumb consumers and optimising consumers). We specify a total of five consumption rules and categorise subjects according to which rule best describes their behaviour. We find that the share of subjects whose behaviour is best described by optimising or near optimising behaviour increases over the course of the experiment, from about 40% to about 70%. Moreover, analysing how people switch between the different consumption rules, we find that subjects switch increasingly less to rules that theoretically violate Ricardian Equivalence in later periods compared to early periods. This implies that subjects learn by adjusting their consumption strategy.

Splitting the sample by consumption rules employed, and assessing the impact that taxation has on each of the subsamples individually, we find that the reaction is much smaller for rules that theoretically imply Ricardian Equivalence compared to rules that do not imply Ricardian Equivalence.

Moreover, the magnitudes of the reaction to taxation appear to be decreasing over rounds for subjects who employ rules that imply Ricardian Equivalence, indicating learning, but remain constant for subjects who employ rules that do not imply Ricardian Equivalence. However, due to the small sample size after this split of the sample, these results should be taken with a grain of salt.

While more research is necessary to quantify differential learning, this provides first evidence that subjects learn differently which is reflected in their choice of consumption strategies.

³⁴ Note that the interpretation is based on a set of subjects assigned to on the round level. Alternatively, one could classify subjects once, according to the consumption rule that minimises the RMSE over all eight rounds. Then the extended specifications can be run for each rule of this classification. We instead decided to report the classification on the round basis, because we find that subjects switch consumption rules quite frequently, as is illustrated in [Table 7](#). However, the results are similar and reported in [Table C.3](#) in the Online Appendix.

These findings have implications for economic theory: Our results imply that models which incorporate a share of people who do not comply with the Ricardian Equivalence may be more realistic than models that assume Ricardian behaviour throughout.

Finally, a word of caution might be in order: In our experiment, subjects get to play eight experimental life cycles, while in reality, individuals typically only observe one life-cycle. Hence, the learning observed in our experiment might overestimate what could be observed outside of the laboratory. Even so, only some of our subjects appear to learn to comply with Ricardian Equivalence in the course of the experiment. The observed failure of the Ricardian Equivalence proposition in this experimental context therefore leaves us somewhat pessimistic about the ability of people to learn Ricardian Equivalence outside of the laboratory.

Supplementary material

Supplementary material associated with this article can be found, in the online version, at [10.1016/j.jedc.2017.07.004](https://doi.org/10.1016/j.jedc.2017.07.004).

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Online Supplementary Material

A Optimal Consumption with CARA Preferences

Following [Caballero \(1990, 1991\)](#), assume that optimal consumption follows an AR(1)-Process:

$$c_{t+1} = c_t + \Gamma_t + \nu_{t+1}, \quad (1)$$

Since the income generating process follows a discrete uniform distribution, the error of the consumption process should follow the same distribution. Define the stochastic error as $\nu_{t+1} = \zeta_{t+1}\varepsilon_{t+1}$ with

$$\varepsilon_{t+1} = \begin{cases} 1 & \text{with probability } 1/2 \\ -1 & \text{with probability } 1/2. \end{cases}$$

Where ζ_t is the standard deviation of consumption in period t . From the numerical solution¹, we observe that ζ_t grows between periods t and $t + 1$ in the following way:

$$\zeta_{t+1} = \frac{T - t + 1}{T - t} \zeta_t. \quad (2)$$

We can therefore write:

$$c_{t+1} = c_t + \Gamma_t + \frac{T - t + 1}{T - t} \zeta_t \varepsilon_{t+1}, \quad (3)$$

Now we need to pin down Γ_t . We start from the Euler equation

$$1 = E_t[\exp^{-\theta(c_{t+1}-c_t)}]. \quad (4)$$

Plugging (1) in (4) yields

$$\Gamma_t = \frac{1}{\theta} \log\{E_t[\exp^{-\theta\nu_{t+1}}]\} = \quad (5)$$

$$= \frac{1}{\theta} \log[1/2 \exp^{-\theta\zeta_{t+1}\varepsilon_{t+1}} + 1/2 \exp^{\theta\zeta_{t+1}\varepsilon_{t+1}}] \quad (6)$$

$$= \frac{1}{\theta} \log \cosh[\theta\zeta_{t+1}]. \quad (7)$$

¹We followed [Carroll \(2011\)](#) to obtain the numerical solution.

$$\Gamma_t = \frac{1}{\theta} \log \cosh \left[\theta \frac{T-t+1}{T-t} \zeta_t \right]. \quad (8)$$

Iteration of (1) from t to $t+j$ gives

$$c_{t+j} = c_t + \sum_{i=1}^j \Gamma_{t+i-1} + \sum_{i=1}^j \nu_{t+i}, \quad (9)$$

where $\sum_{i=1}^j \Gamma_{t+i-1} = \sum_{i=1}^j \frac{1}{\theta} \log \cosh \left[\theta \frac{T-t+1}{T-t+1-i} \zeta_t \right]$,

$$\sum_{i=1}^j \nu_{t+i} = \sum_{i=1}^j \frac{T-t+1}{T-t+1-i} \zeta_t \varepsilon_{t+i}. \quad (10)$$

Iteration of (9) from $t+j$ to $T-t$ gives

$$\sum_{j=0}^{T-t} c_{t+j} = (T-t+1)c_t + \sum_{j=0}^{T-t} \sum_{i=1}^j \Gamma_{t+i-1} + \sum_{j=0}^{T-t} \sum_{i=1}^j \nu_{t+i}. \quad (11)$$

The iterated intertemporal budget constraint is

$$\sum_{j=0}^{T-t} c_{t+j} = a_t + \sum_{j=0}^{T-t} y_{t+j} - \sum_{j=0}^{T-t} \tau_{t+j}, \quad (12)$$

where $E_t[\sum_{j=0}^{T-t} y_{t+j}] = y_t + (T-t)y_p$ and $y_p = E[y_t]$.

Therefore, taking expectations gives

$$\begin{aligned} (T-t+1)c_t + \sum_{j=0}^{T-t} \sum_{i=1}^j \Gamma_{t+i-1} + \sum_{j=0}^{T-t} \sum_{i=1}^j \frac{T-t+1}{T-t+1-i} \zeta_t \underbrace{E_t[\varepsilon_{t+i}]}_{=0} \\ = a_t + y_t + (T-t)y_p - \sum_{j=0}^{T-t} \tau_{t+j}. \end{aligned} \quad (13)$$

Solving for c_t gives

$$c_t = \frac{1}{T-t+1} \left(a_t + y_t + (T-t)y_p - \sum_{j=0}^{T-t} \tau_{t+j} - \sum_{j=0}^{T-t} \sum_{i=1}^j \frac{1}{\theta} \log \cosh \left[\theta \frac{T-t+1}{T-t+1-i} \zeta_t \right] \right). \quad (14)$$

From equation (2) we know that

$$\zeta_t = \frac{\zeta_T}{T-t+1}. \quad (15)$$

Since the marginal propensity to consume in the last period is 1, we know that the standard deviation of the consumption process must equal the standard deviation of the income process, $\zeta_T = \sigma_y$. Therefore we can write:

$$c_t^* = \frac{1}{T-t+1} [a_t + y_t + (T-t)y_p - \mathcal{T}_t - \Gamma_t(\theta\sigma_y)], \quad (16)$$

$$\Gamma_t(\theta\sigma_y) = \sum_{j=0}^{T-t} \sum_{i=1}^j \frac{1}{\theta} \log \cosh \left[\frac{\theta\sigma_y}{T-t+1-i} \right], \quad (17)$$

$$\mathcal{T}_t = \sum_{j=0}^{T-t} \tau_{t+j} = \vartheta - \sum_{j=1}^{t-1} \tau_j. \quad (18)$$

B Experimental Literature on Ricardian Equivalence

Table B.1: Experimental Literature on Ricardian Equivalence

Study	Model	Manipulation/Treatments	Ricardian Equivalence holds?
Cadsby and Frank (1991)	Overlapping Generations	A number of different parametrisations (e.g. endowments, periods, utility specifications)	yes
Slate et al. (1995)	Overlapping Generations	Different probabilities of debt repayment	no, if prob. of debt repayment < 1 , yes if prob. of debt repayment = 1
Di Laurea and Ricciuti (2003)	Overlapping Generations	Liquidity constraints, income uncertainty	yes with liquidity constraints and baseline, no with income uncertainty
Adji et al. (2009)	Overlapping Generations	Distortionary and non-distortionary taxation	yes with non-distortionary taxation, no with distortionary taxation
This study	Life Cycle	Difficulty to smooth consumption, pre-announced taxation	no on average, evidence for Ricardian Equivalence and learning for specific consumption rules

Notes: In the overlapping generation models Ricardian Equivalence usually rather depends on whether “participants placed in a particular environment will interact” (Cadsby and Frank, 1991) than whether they use a behavioural strategy that does or does not violate Ricardian Equivalence and to what extent there is learning behaviour. Therefore, in overlapping generation models Ricardian Equivalence depends on different subjects, while in this study Ricardian Equivalence depends only on the subject’s own decisions.

C Additional Tables and Figures

Table C.1: Median Regressions on Deviation from Optimal Consumption.

Constant	1202.9***	(35.04)
Treatment (base: Control):		
dR1	188.7***	(3.93)
dR2	226.5***	(4.79)
Round dummies (base: round 1):		
d _{r.2}	-247.8***	(-5.10)
d _{r.3}	-471.8***	(-9.72)
d _{r.4}	-417.3***	(-8.60)
d _{r.5}	-448.1***	(-9.23)
d _{r.6}	-486.2***	(-10.02)
d _{r.7}	-521.8***	(-10.75)
d _{r.8}	-574.5***	(-11.83)
Interaction of treatment and round (base: Control):		
d _{r.2} × dR1	-96.41	(-1.42)
d _{r.3} × dR1	-58.89	(-0.87)
d _{r.4} × dR1	-274.4***	(-4.04)
d _{r.5} × dR1	-105.3	(-1.55)
d _{r.6} × dR1	-118.7*	(-1.75)
d _{r.7} × dR1	-195.3***	(-2.88)
d _{r.8} × dR1	-96.18	(-1.42)
d _{r.2} × dR2	54.31	(0.81)
d _{r.3} × dR2	18.98	(0.28)
d _{r.4} × dR2	-21.58	(-0.32)
d _{r.5} × dR2	-103.8	(-1.55)
d _{r.6} × dR2	-101.5	(-1.52)
d _{r.7} × dR2	-168.2**	(-2.52)
d _{r.8} × dR2	-167.9**	(-2.51)

Notes: The dependent variable is aggregate absolute deviation from optimal consumption (see equation (8) in the main text). T-statistics are in parentheses. T-statistics and significance levels refer to tests of the hypothesis for which the coefficient of the respective variable is equal to zero; significance levels are * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.2: Fixed Effects Regressions on Observed Consumption.

	Control		Ricardian 1		Ricardian 2		Ricardian 3	
\tilde{y}	1.449**	(8.69)	1.312***	(13.85)	1.256***	(13.20)	1.261***	(13.48)
\tilde{a}	0.782***	(22.38)	0.781***	(23.38)	0.782***	(24.48)	0.782***	(25.66)
$(T - t)\tilde{y}_p$	1.097	(1.48)	1.301	(3.61)	1.104	(3.10)	1.342	(3.70)
$\tilde{\mathcal{J}}$			0.291***	(1.08)	0.314***	(3.42)	0.548**	(2.54)
$\tilde{\Gamma}(\theta\sigma_y)$	-8.904	(-0.66)	-0.444	(-0.06)	-2.729	(-0.43)	0.633	(0.09)
Tax dummies (base: 120):								
d.0.tx			18.20*	(1.84)	35.08***	(4.61)	29.86***	(3.43)
d.240.tx			-41.14***	(-4.13)	-27.19***	(-3.92)	-33.01***	(-2.82)
t	-2.967	(-1.29)	-3.522**	(-2.44)	-2.374	(-1.60)	-2.980**	(-2.15)
t ²	-0.0196	(-0.08)	0.0985	(0.84)	0.0322	(0.27)	0.110	(0.95)
Round dummies (base: round 1):								
d _{r.2}	-23.79	(-1.07)	-11.08	(-0.97)	-9.502	(-0.79)	-15.55	(-1.38)
d _{r.3}	-15.45	(-0.72)	-7.147	(-0.65)	-6.732	(-0.57)	-8.385	(-0.74)
d _{r.4}	-17.37	(-0.74)	-5.050	(-0.42)	-8.538	(-0.69)	-8.426	(-0.69)
d _{r.5}	-10.86	(-0.48)	-2.856	(-0.25)	-5.022	(-0.42)	-5.088	(-0.44)
d _{r.6}	-20.27	(-0.91)	-8.709	(-0.75)	-8.623	(-0.72)	-9.535	(-0.78)
d _{r.7}	-20.88	(-0.93)	-6.252	(-0.53)	-8.851	(-0.74)	-10.90	(-0.92)
d _{r.8}	-24.08	(-1.07)	-6.365	(-0.54)	-9.706	(-0.81)	-13.18	(-1.10)
Constant	-120.6*	(-1.79)	-102.3***	(-4.24)	-84.14**	(-2.12)	-77.11***	(-2.94)
Adjusted R^2	0.639		0.620		0.617		0.629	
Overall R^2	0.526		0.509		0.506		0.516	

Notes: The dependent variable is observed consumption (c_{itr}). T-statistics based on cluster robust (subject level) standard errors are in parentheses. T-statistics and significance levels of the first five regressors refer to tests of the H_1 (see text) that the respective variable is equal to 1, significance levels are * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All other t statistics and significance levels refer to tests of the H_2 (see text) for which the respective variable is equal to zero; significance levels are * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.3: Fixed Effects Regressions on Observed Consumption Across Heuristics.

	Optimal		σ -Zero-Approximation		Constant		Net Income		Cash on Hand	
Tax dummies (base: 120):										
d_0.tx	20.37***	(3.30)	20.27**	(2.33)	22.40	(1.36)	74.72***	(3.73)	39.15*	(1.88)
d_240.tx	-22.95***	(-4.06)	-31.55***	(-3.90)	-23.61	(-1.37)	-59.98***	(-8.05)	-23.49	(-0.78)
Interaction of tax cut and round (base: round 1, 120):										
d_0.tx × d _{r,2}	-3.387	(-0.60)	-10.26	(-1.09)	-8.692	(-0.52)	-22.51	(-1.25)	-0.595	(-0.03)
d_0.tx × d _{r,3}	-6.037	(-0.96)	-3.516	(-0.37)	-12.46	(-0.66)	17.77	(0.57)	-2.562	(-0.11)
d_0.tx × d _{r,4}	-13.57**	(-2.18)	-16.23*	(-1.92)	-19.84	(-1.11)	-20.53	(-0.83)	0.735	(0.03)
d_0.tx × d _{r,5}	-14.22**	(-2.59)	-6.932	(-0.67)	-18.65	(-1.12)	-20.13	(-0.91)	-9.995	(-0.46)
d_0.tx × d _{r,6}	-8.701	(-1.36)	-11.27	(-1.34)	-9.832	(-0.57)	-26.67	(-0.90)	-13.38	(-0.63)
d_0.tx × d _{r,7}	-13.49**	(-2.23)	-17.69**	(-2.25)	-16.37	(-0.97)	-38.16	(-1.38)	-5.654	(-0.26)
d_0.tx × d _{r,8}	-14.13**	(-2.35)	-17.45*	(-1.81)	-18.26	(-1.04)	-30.97	(-1.01)	-9.798	(-0.46)
Interaction of tax increase and round (base: round 1, 120):										
d_240.tx × d _{r,2}	-2.564	(-0.46)	10.22	(1.13)	10.46	(0.98)	-9.687	(-1.12)	-27.01	(-0.90)
d_240.tx × d _{r,3}	7.916	(1.33)	7.323	(1.03)	11.18	(0.63)	18.11	(1.31)	-14.42	(-0.43)
d_240.tx × d _{r,4}	1.275	(0.20)	19.38**	(2.57)	17.09	(0.96)	5.518	(0.51)	-12.59	(-0.38)
d_240.tx × d _{r,5}	4.678	(0.68)	18.17**	(2.58)	18.65	(1.00)	4.735	(0.36)	-13.63	(-0.41)
d_240.tx × d _{r,6}	5.111	(0.80)	11.71*	(1.75)	11.11	(0.59)	12.87	(0.96)	-10.29	(-0.30)
d_240.tx × d _{r,7}	8.369	(1.43)	22.22***	(2.94)	22.46	(1.33)	5.680	(0.59)	-10.82	(-0.34)
d_240.tx × d _{r,8}	13.46**	(2.30)	23.74***	(2.96)	18.28	(0.96)	15.11	(1.57)	-13.69	(-0.40)
Round dummies (base: round 1):										
d _{r,2}	0.164	(0.04)	2.927	(1.08)	9.734	(0.77)	11.84***	(2.85)	-9.174	(-0.81)
d _{r,3}	-0.231	(-0.05)	-2.400	(-0.91)	5.530	(0.62)	3.192	(0.78)	-17.23	(-1.58)
d _{r,4}	5.019	(1.31)	4.628*	(1.69)	8.189	(0.88)	16.18***	(3.74)	-7.491	(-0.61)
d _{r,5}	4.500	(1.23)	2.692	(1.08)	8.128	(0.97)	18.67***	(3.75)	-10.12	(-0.92)
d _{r,6}	0.135	(0.03)	-0.848	(-0.34)	2.128	(0.29)	11.13**	(2.16)	-13.15	(-1.10)
d _{r,7}	4.675	(1.20)	0.892	(0.38)	4.884	(0.63)	16.05***	(2.90)	-10.28	(-0.89)
d _{r,8}	0.863	(0.23)	-3.129	(-0.84)	4.306	(0.57)	12.91**	(2.45)	-13.70	(-1.15)
Constant	-27.65	(-0.85)	-157.7***	(-6.41)	-11.29	(-0.24)	-353.8***	(-5.94)	-68.15	(-1.36)
Additional Controls	✓		✓		✓		✓		✓	
Adjusted R^2	0.202		0.177		0.042		0.283		0.096	
Overall R^2	0.175		0.162		0.0355		0.272		0.0998	

Notes: The dependent variable is observed consumption (c_{itr}). T-statistics based on cluster robust (subject level) standard errors are in parentheses. T-statistics and significance levels refer to tests of the H_0 for which the respective variable is equal to zero; significance levels are * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Full estimation results are available on request.

Table C.4: Strategy Evolution.

Strategy	Unit	Round							
		2	3	4	5	6	7	8	
from σ -Zero-Approximation	(%)	9	7	18	9	15	15	11	
from Optimal Consumption	(%)	18	11	16	16	10	8	11	
from Constant Consumption	(%)	8	18	8	3	17	3	3	
from Fraction of Cash on Hand	(%)	16	12	5	7	6	7	7	
from Fraction of Net Income	(%)	7	8	6	6	6	3	3	
Total Switchers	(%)	58	56	53	41	54	36	35	

Figure C.1: Aggregate Monetary Loss by Treatments in Euro.

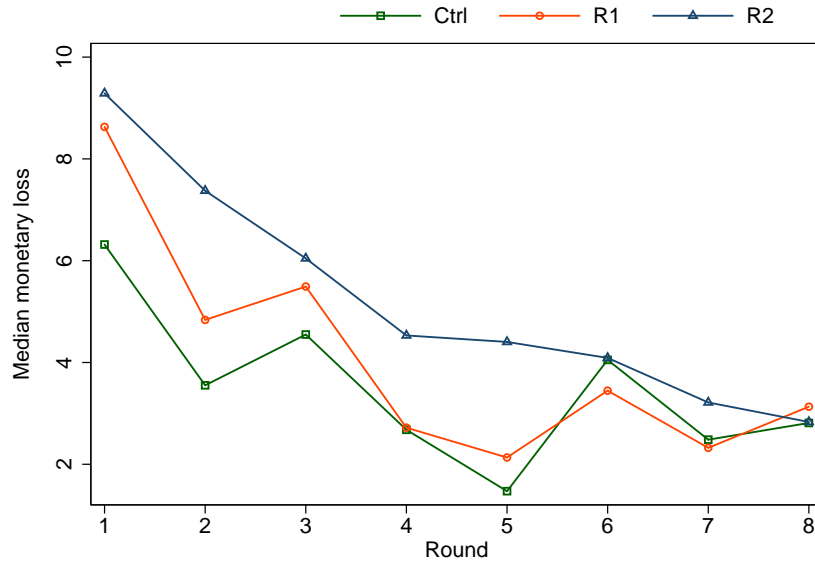


Figure C.2: Aggregate Monetary Loss by Heuristic in Euro.

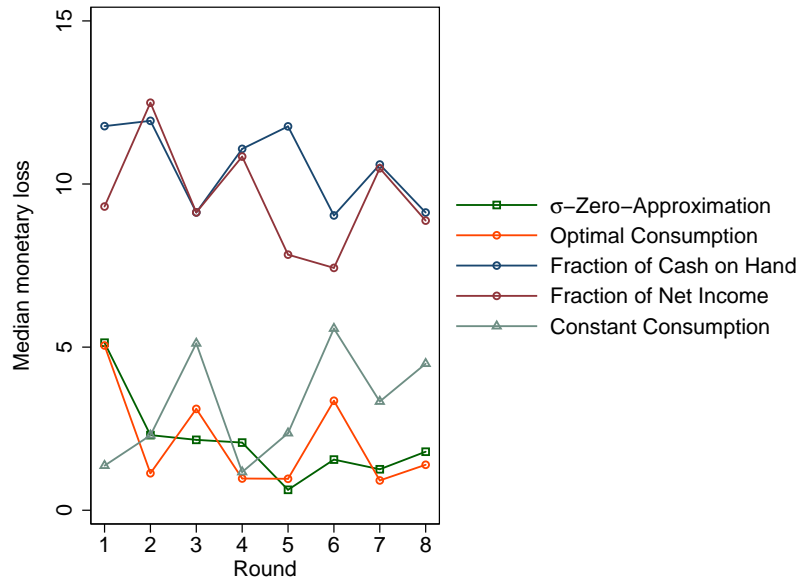
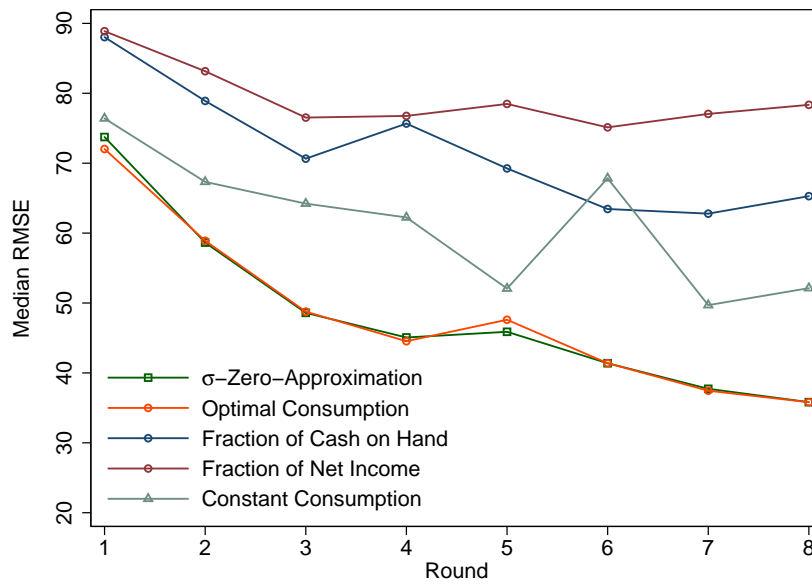


Figure C.3: RMSE.



D Instructions

This section contains the instructions of the experiment.² Subjects in all treatments received the same instructions, except for the treatment Ricardian 3. Differences are indicated by footnotes.

Instructions

Welcome to this experiment!

During this experiment you are not allowed to use electronic devices or to communicate with other participants. Please only use programs provided for this experiment. Please do not talk to other participants. If you have a question, please raise your hand. We will then come to you and answer your question individually. Please do not ask your question out loud. If your question is relevant for all participants, we will repeat your question out loud.

Overview. First you will have time to read the instructions. After that we will go through the instructions together, and you will complete a quiz in order to make sure you understood the instructions. The experiment consists of 8 rounds, each of which consists of 25 periods. The duration of the experiment is around 1.5 hours. Instructions, quiz, and a questionnaire will take around 30 minutes. The remaining hour is dedicated to the actual experiment. After the last round, your experiment payoff will be displayed. Please raise your hand when you have finished the last period. You will then be handed a short questionnaire. After filling out the questionnaire, please raise your hand again. You will then receive your experiment payoff in the adjacent room.

Your task is to decide in every period how many *points* you want to purchase. The sum of all points purchased in one round is that period's result. Your payoff depends on the results from two randomly drawn rounds.

Income, Savings and Wealth. In every period you obtain a certain *income*, denoted in the experimental currency "Taler". From this income you have to pay a certain amount of taxes to the government. Your task is to choose how many Taler to spend in order to purchase points. Thereby you (implicitly) also choose how many

²The instructions printed here are a translation of the original German version.

Taler you want to save or borrow. We call your income minus spending and taxes in one period *savings*.

Your *wealth* in the first period of every round is 1,000 Taler (initial wealth). The wealth in every later period equals the wealth of the previous period plus savings (=income-spending-taxes) of the previous period.

Please note that the sign of the savings can be either positive or negative. If you decide to spend fewer Taler than you have as income minus taxes, your savings have a positive sign. In this case your wealth in the next period is your wealth in this period plus the absolute amount of savings in this period. Should you decide to spend more Taler than you have as income, your savings have a negative sign. In this case your wealth in the next period is your wealth in this period minus the absolute amount of savings.

Example: assume your income in one period is 50 Taler and you have to pay 10 Taler in taxes. If you spend 30 Taler to purchase points, your savings are 10 Taler. In case you instead spend 70 Taler with the same income, your savings are -30 Taler. In the first case your wealth in the next period is the wealth in this period plus 10 Taler. In the latter case your wealth in the next period is this period's wealth minus 30 Taler.

Your wealth may take positive or negative values as well, depending on whether your savings from previous periods plus your initial wealth were positive or negative. In the last period, your wealth plus income minus taxes will be spent automatically in order to purchase points. This implies that the sum of Taler spent in all periods of one round equals the sum of income obtained in all periods of this round minus the sum of all taxes paid in this round. In other words: you may spend more or less than your income in one round. However, over one round, the sum of income plus initial wealth always equals the sum of Taler spent plus the sum of all taxes.

Determination of Income and Taxes. Your income is randomly determined. In every period, your income can take the values of either 250 Taler or 120 Taler. Both values occur with the equal probability of 50%. It is very important to understand that income is truly randomly determined. The value the income takes in one period does not depend on the values it had in previous periods or how you behaved in previous periods.

The government has fixed costs of 120 Taler in every period, which you have to finance through taxes. This implies that the government collects a total of $120 \times 25 = 3000$ Taler from you in the course of one round. The government is free to collect

more or less than 120 Taler in taxes in any period.³ Before you decide how much to spend in every period you learn the amount of taxes the government collects from you in the respective period.

Taler and Points. Your task to decide in every period how many Taler you want to spend in order to purchase points. Taler are transformed to points as follows:

$$\text{Points} = 338 \times \left(1 - e^{-0.0125 \times (\text{chosen amount of Taler})}\right)$$

A graph of this function, as well as a table with relevant function values is attached to the instructions.⁴ Please note that the above function is defined in the positive as well as the negative domain. If you choose to spend a negative amount of Taler, you receive a negative amount of points. In this case you “sell” points and gain Taler. Should your wealth plus income in the last period be negative, you will have to automatically sell points in order to make sure that your Taler account is balanced.

Payoff. For your participation you will receive a fixed amount of 5 Euro. Additionally you will receive an amount that depends on the results of two randomly drawn rounds. This amount is calculated as follows:

$$\text{Payoff in Euro} = \frac{(\text{Result1} - 5000) + (\text{Result2} - 5000)}{100}$$

where Result1 is the first randomly drawn result and Result2 is the second randomly drawn result.

Example: suppose the first randomly drawn result is 5500 points and the second randomly drawn result is 6000 points. Your payoff is then:

$$\frac{(5500 - 5000) + (6000 - 5000)}{100} = \frac{1500}{100} = 15 \text{ Euro.}$$

³This sentence was replaced in treatment Ricardian 3 by: “In periods 2, 4 and 6, the government grants you a tax relief. In these periods you do not have to pay any taxes. In periods 18, 20, and 22 the government raises taxes: in these periods you have to pay 240 Taler in taxes. In all other periods, the government taxes you with 120 Taler.”

⁴Omitted here.

Should the payoff calculated according to the formula above fall below 0 Euro this will be counted as 0 Euro. In any case you will receive the fixed amount of 5 Euro. This implies that you will earn *at least* 5 Euro.

Quiz and Questions. You will now be asked to answer a short quiz regarding the contents of these instructions. In case you have questions after that, please raise your hand. An experimenter will then come to you and answer your question.

Figure D.1: Screenshot of the Experimental Interface.
Source: Own interface based on z-Tree.

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Runde: 1
Periode: 11

Ihr Einkommen in dieser Periode (in Taler): 250.00
 Ihre Steuern in dieser Periode (in Taler): 120.00
 Einkommen - Steuern (in Taler): 130.00

Ihr Vermögen in dieser Periode (in Taler): 716.00
 Einkommen + Vermögen - Steuern (in Taler): 846.00

Bitte geben Sie den Betrag ein den Sie ausgeben möchten um Punkte zu erwerben (in Taler):

OK

Sie sparen in dieser Periode (in Taler): 130.00
 Ihr Vermögen in der nächsten Periode (in Taler): 846.00
 Ihre Ausgaben in dieser Periode (in Taler): 0.00
 Erworbene Punkte: 0.00

Weiter

Periode	Runde	Einkommen	Steuern	Summe der gezahlten Steuern	Vermögen	Ausgaben (Taler)	Erworbene Punkte	Punktstand
1	1	120.00	0.00	0.00	1000.00	98.00	238.71	238.71
2	1	120.00	0.00	0.00	1022.00	100.00	241.16	479.87
3	1	120.00	0.00	0.00	1042.00	100.00	241.16	721.03
4	1	120.00	120.00	120.00	1062.00	98.00	238.71	959.74
5	1	250.00	120.00	240.00	964.00	100.00	241.16	1200.90
6	1	250.00	120.00	360.00	994.00	105.00	247.03	1447.93
7	1	120.00	120.00	480.00	1019.00	110.00	252.54	1700.47
8	1	120.00	120.00	600.00	909.00	110.00	252.54	1953.01
9	1	250.00	120.00	720.00	799.00	108.00	250.38	2203.39
10	1	120.00	120.00	840.00	821.00	105.00	247.03	2450.42

E Quiz

Please indicate your answers to the following questions next to “your answer”. After you have completed the quiz, we will go through the questions together. Please fill in the correct answers under “correct answer”.

Income

a) True or false: your income in period 10 is either 100 or 230.

Your answer: Correct answer:

b) What is the probability of your income to be 250 Taler in period 15?

Your answer: Correct answer:

c) Suppose you were “lucky” in period 15, and received 250 Taler. How high is the probability to be lucky in the next period and receive 250 Taler?

Your answer: Correct answer:

Wealth and Savings

Suppose your wealth in some period is 50 Taler. Your income is 250 Taler. You decide to spend 140 Taler to purchase points.

a) What is the amount you save this period?

Your answer: Correct answer:

b) What will be our wealth in the next period?

Your answer: Correct answer:

Suppose you decide instead to spend 300 Taler.

c) What is the amount you save this period?

Your answer: Correct answer:

d) What will be your wealth in the next period?

Your answer: Correct answer:

Points and Taler

- a) Have a look at the enclosed table. Suppose you wish to spend 300 Taler. How many points will you receive?

Your answer: Correct answer:

- b) Suppose you want to spend -10 Taler. How many Taler do you get in return?

Your answer: Correct answer:

- c) Suppose you want to buy 286.17 points. How many Taler do you have to spend?

Your answer: Correct answer:

- d) Suppose you want to buy 105.7 points. How many Taler do you have to spend?

Your answer: Correct answer:

- e) Suppose you want to “sell” 45 points (i.e. buy -45 points). How many Taler do you get in return?

Your answer: Correct answer:

- f) Suppose your wealth is -50 Taler in the last period of one round. Your income is 250 Taler and the taxes are 100 Taler. How many points do you purchase automatically in this period?

Your answer: Correct answer:

Taxes

You are in period 10. Suppose, the government has collected a total of 1000 Taler as taxes so far. How much taxes does the government have to collect from you in the remaining periods?

Your answer: Correct answer:

Your Payoff

Suppose your period earnings in rounds 1 to 8 are: 5500, 5200, 4300, 6000, 6300, 5400, 3000 and 5700 points respectively.

- a) The random draw determines rounds 1 and 5 to be payoff relevant. How many Euro do you earn? Please include your fixed payment of 5 Euro.

Your answer: Correct answer:

- b) The random draw determines rounds 3 and 4 to be payoff relevant. How many Euro do you earn? Please include your fixed payment of 5 Euro.

Your answer: Correct answer:

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